

## DISCUSSION ON 'TROPOSPHERIC PROPAGATION BEYOND THE HORIZON—I' AT THE SYMPOSIUM, 28TH JANUARY, 1958

**Dr. R. L. Smith-Rose:** The papers in this session describe the progress made in a field which has been under investigation for over a quarter of a century. As Mr. Isted indicates, the subject was investigated by Marconi and other workers in the late 1920's and early 1930's. Mr. Kitchen refers to the work of M. Jouaust in France, who was probably one of the first experimenters to suggest that the variations in signal strength in the transmission of metre waves were due to fluctuations in atmospheric conditions. But even before that, in this country T. Y. Baker has indicated the theoretical basis of atmospheric refraction. In 1929, also, R. M. Shmakov described experiments conducted in the neighbourhood of Moscow, in which he showed that wavelengths of a few metres could be propagated well beyond the horizon distance. Furthermore, commercial communication links were being established in different parts of the world, e.g. between France and Corsica in 1929, and between islands of the Hawaiian group in 1931.

So much for the history of the subject. The present papers are, I think, divisible into two groups: one comprises those represented by Messrs. Rowden *et al.*, in which a large mass of experimental facts has been collected together systematically. It is this type of material that forms the basis of international discussion on the engineering possibilities of communication at the frequencies in question; and I should like to take this opportunity, as International Chairman of the C.C.I.R. Study Group concerned with this matter, of thanking all my British colleagues in the B.B.C., Post Office, the Admiralty and D.S.I.R. for carrying out what is a very arduous and extensive amount of work which does not look very much when it is reduced to the few curves on which international agreement is reached. The London C.C.I.R. curves of 1953, which are reproduced as Fig. 1 of Mr. Jowett's paper, have now been revised; they are at present in course of circulation among the national administrations in the revised form, taking account of all the increase in knowledge which is incorporated in these papers.

The other type of paper gives us the collection of the experimental facts followed by an attempt to explain them in the light of the prevailing meteorological or other conditions. Here I am very interested to see two opposite points of view: Messrs. Starkey *et al.* see no reason to assume that the mechanism of tropospheric propagation is due to any other than reflections from a region which may be one or more inversion layers, or may be a layer at the tropopause; on the other hand, Messrs. Kitchen *et al.* state that this phenomenon is not a sufficiently permanent and continuous feature of the atmosphere for exploitation in communication systems operating in the trans-horizon region. It is not quite clear why the authors feel so strongly on these different points of view; because bearing in mind the circumstances under which the measurements were made—in an aeroplane in one case and in a ship at sea in the other—one might expect variations of  $\pm 5$ –10 dB in many cases. Thus there is not such a great difference, for example, between Fig. 3 of the paper by Kitchen *et al.* and Figs. 1 and 2 of that by Starkey *et al.*

Some of the papers refer to a paper by G. K. Batchelor, of Cornell University. This, so far as I and my colleagues are

concerned, has been until this afternoon a classified report, but apparently it is available to certain of these authors; from the regard they accord it, this would appear to be a very important paper, and I wonder whether steps cannot be taken to secure its publication.

I did not quite understand the full implication of the contribution by Messrs. Buxton and Felix, but I should like to know whether, by compressing the frequency band and so securing the advantages described, we are not also limiting the amount of information that can be conveyed over the communication link. Do we not reach the limit of this technique, for example, in the Janet system, in which perhaps for nine-tenths of the time no information whatever is passing, and then when a burst comes along a lot of information is released at very high speed? What matters, of course, is the average amount of information passed per hour.

**Dr. J. S. McPetrie:** I was very interested in the point raised by Dr. Smith-Rose, because I feel that we have turned right round in our thinking. When Marconi did his beyond-the-horizon v.h.f. experiments the results were assumed to be due to reflection; then we assumed scattering and forgot about reflection. Now Messrs. Starkey *et al.* put forward the reflection theory again, and I feel that Kitchen *et al.* are rather beating a retreat in coming up with both.

In this connection I feel the title of Mr. Isted's paper is slightly misleading, because the main thing Marconi did beyond the horizon was his long-wave propagation tests across the Atlantic. I think that v.h.f. should be included in the title.

With regard to the paper by Starkey *et al.*, we have now equipped a Canberra at R.A.E. in order to fly above the tropopause, and I should like to ask the authors for a progress report. Once we have performed the experiment above the tropopause, if the reflection theory is right, there should be less reflection at layers and so the results should approach more nearly to those expected from normal diffraction.

**Mr. H. Stanesby:** Mr. Isted writes about the impressive acts of faith of Marconi. We in the Post Office are not without faith ourselves, and just before the 1939–45 War we were convinced of the possibility of establishing v.h.f. radiocommunication across the North Sea from the Shetlands to Norway. As a result, a few days' tests, which were stopped by the outbreak of war, established that *prima facie* there was a good chance of doing this. Some time after the war the work was resumed, and at the beginning of 1954, jointly with the Norwegian Administration, we brought into service a multi-channel telephone system operating at approximately 40 Mc/s between the Shetlands and Bergen. To-day this would be described as a tropospheric-scatter system. High-gain rhombic aerials are used at both ends, 850 and 1800 ft above sea level in the Shetlands and at Bergen respectively, and the distance between the terminals is 226 miles. Twelve telephone circuits are provided, three on each of four 300-watt carriers; and although the power is low as judged by present-day standards, the performance is reasonably good, a 26 dB signal/unweighted-noise ratio being exceeded for about 95% of the time.

**Mr. F. A. Kitchen:** There appears to have been some loose

thinking on the problem of long-distance metre-wave propagation. At present I find it difficult to be specific about the effective mechanism under general atmospheric conditions, for our experience shows that a variety of mechanisms are involved. Some of the time we have observed strong partial reflections from elevated inversions—a well-known phenomenon investigated extensively during the war; at others we have identified a composite signal consisting of a number of weak reflections from discrete inversions which could give rise to the mechanism described by Starkey *et al.* Finally we have detected purely random scattering from atmospheric turbulence.

I think that this whole range of mechanisms can occur. But for what proportion of time? Is one mode predominant? Is it only during a violent gale that we observe turbulence scatter at metre wavelengths, and do we generally suffer from the effects of reflections from inversions in these latitudes? At this moment I do not know, but we are proposing to set up a point-to-point link using the 200 Mc/s equipment over a range of some 250 n.m. for long-term observations on the time variation of signal character at these wavelengths.

I should like to point out one important aspect of inversion reflections which has perhaps been overlooked in this session. In the presence of a strong inversion, partial reflection from it at metre wavelengths might well affect the illumination of the scattering volume. On the one hand there results a coherent component at the receiver input, and on the other the scatter contribution might be reduced. Perhaps this has something to do with the differences in interpretation.

The origins of Dr. Batchelor's report were mentioned. It has been referred to previously in the literature\* and is an unclassified report. We understand from correspondence between the late Dr. Megaw and Dr. Batchelor that it is available from Cornell University.

**Dr. J. A. Saxton:** I challenge the claim by Messrs. Kitchen *et al.* that, from metre-wave experiments, they have incontrovertible evidence of having seen signals due to turbulent scattering. If this is deduced solely from the nature of the signal records—which must be the case, since we do not have enough detailed information concerning the meteorological conditions in the atmosphere to allow us to do anything else—I suggest that, although it is possible to say what kind of field-strength recordings would result from combining specified types of signal, it is impossible to reverse the process and deduce unique propagation conditions from the fading characteristics of the records. It is well known that only four or five randomly phased components are needed to produce a result which is indistinguishable from a Rayleigh-distributed signal; although this, the authors suggest, is evidence of the large number of components associated with turbulent scattering. It is therefore impossible to deduce the propagation mechanism purely from a knowledge of the characteristics of the received signal; and to solve this problem we must have much more direct information concerning the fine structure of tropospheric refractive-index variations.

**Dr. H. G. Hopkins:** It seems rather unlikely that the elevated discontinuity layers discussed by Messrs. Starkey *et al.* will be precisely parallel to the earth's surface. What departures from parallelism occur, and are they of sufficient magnitude to affect the limiting ranges for the rays shown in Figs. 1 and 2?

**Mr. E. Sofaer:** Mr. Jowett produces curves in Fig. 3 which fit the measured points remarkably well when plotted against angular distance, but I would stress the very arbitrary nature of this parameter. In Fig. 5(b) he shows the transmitter on the right-hand side, and its horizon somewhat to the left; he also

shows the intersection of horizons, to demonstrate the angle defining angular distance. One can imagine the transmitter being situated on any of the three major peaks that lie between the horizon and its present position, and if the aerial heights are appropriate, the distance between the transmitter and the receiver may be halved and the field strength increased without changing the angular distance. It is therefore a very arbitrary parameter against which to plot measurements.

In Section 3.1 a sentence reads: 'Attention is focused . . . upon terminal height.' I think we need a proper definition of terminal height. If we mean the physical height, say above sea level, the statement is correct, but an effective height might be a more realistic quantity. Consider a profile where the transmitter is on one hill and the horizon is another hill, and somewhere between a ray is reflected off the ground to the horizon. A line representing this ray, drawn from the horizon through the reflection point passes through the transmitter image directly beneath the transmitter. Let the distance between the transmitter and its image be  $TT'$ . This length divided by the distance between the transmitter and the horizon gives an angle which could represent terminal height; if the ground between the transmitter and the horizon is low,  $TT'$  is large and a large angle results; but if it is high,  $TT'$  is small and the angle is small. I think this is a more realistic way of describing terminal height, and that it would give better correlation.

**Mr. J. K. S. Jowett:** Messrs. Kitchen *et al.* refer to the sudden change in rate of signal attenuation observed at 203.5 Mc/s at about 20 miles beyond the horizon. This phenomenon is also shown in Figures contained in this and other papers concerned with overseas transmission both at very high and much higher frequencies. It is generally accepted that it occurs at the point of transition from the diffracted field to the field set up by tropospheric influences. This sharp change of slope has, so far as I know, never been observed on overland measurements in this country. The reason is presumably that the effects of terrain irregularities are such as to mask any sharp transition, which can readily be observed only by range measurements over sea or perhaps over very smooth ground. It is of some interest to note that empirically-derived overland propagation curves are unlikely to reveal this transition, which nevertheless would probably occur in the case of range tests over smooth overland paths. The distance at which the transition occurs would nevertheless probably change to some extent from day to day in accordance with any large-scale variations in refractive-index gradient.

**Major W. V. G. Fuge:** With regard to the question of a grazing ray, what will be the effect of a large rise and fall of tide?

**Mr. G. A. Isted:** During the 1939–45 War, radio communication between R.A.F. fighters and ground control was carried out at frequencies just above 100 Mc/s. Contact between them was frequently lost, but experience showed that contact could often be regained, not by the fighter increasing its height in order to extend its horizon, but by reducing height considerably. This suggested the presence of marked atmospheric discontinuities capable of limiting radio-wave propagation to heights below them. Some controlled experiments which were carried out during the war between a 200 Mc/s pulsed radar transmitter situated near Great Baddow and an aeroplane, confirmed this view. The primary objective of the experiment was the investigation of the height/gain situation at distances of the order of 150 km from the transmitter, with the view to predicting the performance of radar equipment for intercepting enemy aircraft.

The procedure adopted for the investigation was to fly an aeroplane, equipped with suitable signal-level measuring

\* MEGAW, E. C. S.: 'Fundamental Radio Scatter Propagation Theory', *Proceedings I.E.E.*, Monograph No. 236 R, May, 1957 (104 C, p. 441).

apparatus, in level flight along a 20 km leg broadside to the transmitter and then spiral up 500 ft to the return leg; this procedure continued in steps of 500 ft up to a total height of 10 000 ft. Reliable measurements of signal level were made along each leg and rather less reliable ones during the spiral ascent or descent to the next leg. On one particular occasion in February, 1944, the signal was lost soon after the spiral climb from 7 000 to 7 500 ft commenced; nothing more was heard of the signal up to a height of 10 000 ft. However, during the spiral descent between 7 500 and 7 000 ft the signal reappeared again at good strength; it was estimated at the time that the signal cut-off must have occurred over a height interval of only 150 ft. Subsequent analysis of the relevant meteorological data showed that a widespread violent temperature inversion was present at the time of the radio observation at a height of 7 000 ft.

These results serve to emphasize the magnitude of the effects caused by some of these atmospheric discontinuities, and show clearly that from time to time they must exert a strong influence on long-distance propagation in the troposphere.

**Brigadier E. J. H. Moppett:** For some time we have had in operation in Pakistan an 80 Mc/s multi-channel radio-relay system 535 miles long with 14 repeaters. The transmitter frequencies are repeated every three relays. We have found occasions when conditions exist that permit the system to operate with three repeaters switched off. Such conditions can last for hours, but they are not sufficiently persistent or predictable to be of commercial value.

Scatter communication is generally understood to be a brute-force method using substantial e.r.p. to maintain commercial-grade communication over long distances. Neither our experience in Pakistan nor listening at long range to B.B.C. transmitters radiating small e.r.p. in a given direction can be regarded as true examples of scatter.

**Mr. A. Bickers:** In the equipment described by Messrs. Buxton and Felix, if the band-pass filter following the mixer has a variable bandwidth I agree that a considerable reduction of threshold level can be achieved. This would be particularly advantageous in telegraphy, while another advantage which will result from the feedback system is the reduction of inter-modulation distortion.

Another method of reducing the system threshold level, applicable to diversity, is to combine the signals both before and after final detection. For example, in a dual-diversity system, both carrier and noise from the two i.f. amplifiers may be combined, after correction for any carrier phase displacement, to form a third 'path' with a carrier/noise ratio and threshold level which will be greater than the carrier/noise ratios of the other two paths. This improvement will approach 3 dB when the two signals are equal. Thus the system threshold level, which is determined by the threshold level of the better path, may be improved by a value approaching 3 dB.

**Mr. G. Millington:** Carroll maintains that long-distance tropospheric propagation can be attributed to partial reflections in a continuously graded atmosphere. He has used a model in which the refractive index decreases linearly up to the height at which it becomes unity, above which it remains constant. His conclusions are valid for the model he has chosen, the discontinuity of gradient of refractive index at this height giving rise to enough partial reflection to account for a large number of propagation modes with low attenuation. If this discontinuity is removed by using a model in which the refractive index decreases asymptotically to unity sufficiently gradually, the low attenuation disappears.

Actually the troposphere is not wholly horizontally stratified, and layers may be broken up into relatively small regions which in the limit are comparable in size with the 'blobs' of the turbulence theory. The distinction between reflections from layers and scattering from turbulent centres may therefore not be as definite as is sometimes supposed.

The C.C.I.R. curves to which reference has been made were based on measurements made in the Eastern States of America, in the United Kingdom and in certain regions of Northern Europe. As Brigadier Moppett has pointed out, measurements in tropical countries reveal a very different picture. We have some results of tests made in the Persian Gulf which show that quite regularly there is a temperature inversion over land at night and the signal is greatly increased, even exceeding the free-space value at times. Use can be made of this fact in planning long-distance links, while it has obvious disadvantages where frequency sharing is concerned.